

# The Role of Contingency in Classical Conditioning

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The assumption that classical conditioning depends on a contingent relation between the conditioned stimulus (CS) and the unconditioned stimulus (US), which was proposed some decades ago as an alternative to the traditional contiguity assumption, still is widely accepted as an empirical generalization, if no longer as a theoretical postulate. The first support for the contingency assumption was provided by experiments in which occasional CS-US pairings produced no response to the CS in random training—i.e., training in which the probability of the US was the same in the presence and absence of the CS. Those early experiments, the results of which too often are taken at face value, are reconsidered along with various later experiments that show conditioning, both of the CS and its context, in random training. The evidence suggests that CS-US contingency is neither necessary nor sufficient for conditioning and that the concept has long outlived any usefulness it may once have had in the analysis of conditioning.

Some Sidman avoidance experiments with dogs by Rescorla and LoLordo (1965) gave rise in the late 1960s to what proved to be an extremely attractive theory of classical conditioning. The seminal finding was that a conditioned stimulus (CS) seemed to become inhibitory in explicitly unpaired presentation with an unconditioned stimulus (US), a procedure long used in conditioning experiments to control for nonassociative effects of the stimuli on the assumption that it produced no learning. If a CS paired with a US acquired excitatory properties and a CS explicitly unpaired with a US acquired inhibitory properties, then presentations of CS and US “truly random in time, as well as in order [might] serve as a better control procedure” (Rescorla & LoLordo, 1965, p. 412). Implicit in this methodological proposal, itself not entirely new (Jensen, 1961; Prokasy, 1965), was a theory of conditioning that Rescorla (1968) soon began to distinguish from the traditional contiguity or “pairing” theory:

The notion of contingency differs from that of pairing in that it includes not only what events are paired but also what events are not paired. As used here, contingency refers to the relative probability of occurrence of US in the presence of CS as contrasted with its probability in the absence of CS. The contingency notion suggests that, in fact, conditioning only occurs when these probabilities differ. (p. 1)

Support for the new theory was found in a Sidman avoidance experiment (Rescorla, 1966) with three groups of dogs separately exposed to paired, explicitly unpaired, or random presentations of CS and shock. The CS, which increased avoidance

responding in the paired group and decreased avoidance responding in the explicitly unpaired group, had no effect on avoidance responding in the random group even though it had been paired with the US as often for the random group as for the paired group. Further support for the contingency theory was found in conditioned-suppression experiments with rats (Rescorla, 1968). With the probability of shock in the presence of the CS— $p(\text{US/CS})$ —held constant, suppression decreased (i.e., the CS seemed to become less excitatory) as the probability of shock in the absence of the CS— $p(\text{US/noCS})$ —increased from a value of zero, and the CS was entirely without effect when the two probabilities (although substantial) were the same.

These experiments left much to be desired. As Estes (1969) was quick to complain, tests with the CS were made only after five sessions of off-baseline random training (i.e., training in which there was no opportunity for instrumental responding). Because conditioned suppression could be demonstrated under suitable circumstances after a single pairing of CS and shock, Estes (1969) speculated, control of performance by the CS might have developed earlier and then been “somehow eliminated” (p. 180). Just such results soon were reported by Rescorla (1972), who found differential responding to the CS after one and three, although not after six, sessions of random training. The absence of differential responding to the CS, Estes went on to suggest, might mean not that the animals failed to anticipate shock in its presence, but only that they had “learned to anticipate shock equally in [its] presence and absence”—that contextual stimuli (“the stimuli characterizing non-CS intervals”) also had been conditioned (p. 180). In an effort to minimize contextual conditioning in random training, Rescorla (1972) signaled all shocks given in the absence of the CS with another salient stimulus. The animals responded differentially to the CS even though the probability of shock still was the same in the presence and absence of the CS, from which Rescorla concluded that “it is not simply the relation between the CS and US which generates the absence of conditioning in the uncorrelated procedure [but] the conditioning of other stimuli present in the situation” (1972, p. 26).

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At the same time, more persuasive challenges to contiguity theory were being considered that were no less challenging to contingency theory. If overshadowing (Pavlov, 1927), blocking (Kamin, 1969), and various other cue-validity phenomena (Wagner, 1969), which commonly were thought to require some sort of informational interpretation, could not be accounted for in terms of contiguity alone, neither could they be accounted for in terms of contingency alone. Both in overshadowing and in blocking, the conditioning of one stimulus was impaired by the presence of another that did not reduce its positive correlation with the US. In a cue-validity experiment by Rescorla (1972), with a light-tone compound reinforced on some trials and the light alone on others, differential responding to the (redundant) tone appeared and then tended to disappear as training continued, although its correlation with the US did not change.

As an alternative to contiguity theory, contingency theory was open to the further objection that the events to be correlated had first to be registered. Dissatisfaction with the idea that animals "take in large blocks of time, count up numbers of US events, and somehow arrive at probability estimates" was expressed early by Rescorla (1969, p. 84), who began to think again about how animals might "bring together the effects of events separated in time in such a way as to permit all learning to depend on events occurring closely in time" (p. 88). The same dissatisfaction was expressed by Wagner (1969), and the two of them together, building on a proposal by Kamin (1969), invented a new contiguity theory that accounted for many of the results challenging the old. In their interpretation of those results, an important role was assigned to the conditioning of contextual stimuli, which of course would have been possible before. The critical difference was that the old independence assumption, according to which the components of a compound stimulus gain and lose strength independently with reinforcement and nonreinforcement (Hull, 1950; Spence, 1936), was replaced in the new theory with the assumption of shared associative strength (Rescorla & Wagner, 1972).

Although contingency theory was renounced by Rescorla himself in 1972, the assumption that contingency is important for conditioning continued to find wide acceptance even as contradictory evidence continued to mount. In any consideration of why that should be, a good deal of weight surely must be given to the influence of Rescorla's 1966 and 1968 results, which often are taken even today as *prima facie* support for the assumption. "Up until 1967 [sic] it was believed that presentation of CS and US contiguous in time would result in conditioning," Schwartz (1989) writes in the latest edition of his *Psychology of Learning and Behavior*. "However, Rescorla's experiments demonstrated that . . . the organism computes probabilities . . . and the outcome of this computation determines whether a potential CS will actually be an effective CS" (p. 110). In *Learning: An Introduction to the Principles of Adaptive Behavior*, Staddon and Ettinger (1989) teach that "the amount of conditioning to a stimulus in a Pavlovian conditioning situation is directly related to the degree of contingency between the stimulus and the unconditioned stimulus," pointing to Rescorla's 1968 results as "an especially clear example of this relation" (p. 94).

In a recent review in *Nature* of the revision of *Stevens' Hand-*

*book of Experimental Psychology*, Sutherland (1989) enthuses about a chapter by Hearst (1988) that features Rescorla's 1968 results together with those of some supposedly analogous work on appetitive conditioning in pigeons (Hearst, Bottjer, & Walker, 1980). The chapter shows, Sutherland writes, that "animal learning, currently a neglected field, has taken a dramatic step forward. It has been found that the formation of an association between two stimuli . . . depends not merely on how frequently one is followed by the other, but on the second stimulus occurring more frequently after the first than in its absence," adding gratuitously in the spirit of the times that "this makes good sense. Animals, and people, need to learn about stimuli that predict an event better than other stimuli, not merely about random connections between the members of a pair of stimuli" (p. 418). In this loose way of thinking, the renewed popularity of which parallels that of the contingency notion itself, is perhaps to be found another reason for the surprising viability of the notion: Complacent speculation about adaptive value takes the place of critical analysis.

The textbooks routinely provide homely illustrations of the "obvious advantages" of being able to learn about "relationships among stimuli"—so "variable and uncertain in the real world"—which "Rescorla's studies show that animals can learn about" (Mazur, 1986, pp. 76–77). The problems of rabbits threatened by predators have been considered by Mazur, and of hunters in pursuit of deer by Domjan and Burkhard (1986). Schwartz (1989) considers weather prediction. Staddon and Ettinger (1989) sow what must be a good deal of misunderstanding in their consideration of fear of dentists. They write of the dentist whose signal of impending pain becomes excitatory because it is positively correlated with pain, of the dentist whose signal becomes inhibitory because it is negatively correlated with pain, and then:

More common perhaps is the dentist whose signal falls on the diagonal line in our contingency space; thus, pain is equally likely whether the dentist signals it or not. It is no wonder that visits to the dentist make us anxious! (p. 96)

The assumption that contingency is important for conditioning continues to be prominent in the journals as well as in the textbooks. A search of the Social Sciences Citation Index in mid-1989 turned up a total 136 references to Rescorla's 1968 paper in the years since its publication, with 50 of them dating from 1986. The citations are inappropriate on occasion, as when Csányi and Lovász (1987) imply that the paper has something to do with overshadowing, or Flaherty and Rowan (1988) that it bears on their hypothesis that rats learn "the contingent relationship between the two solutions" (p. 48) in experiments on anticipatory contrast. Typically, however, the paper is cited to document the claim that conditioning is sensitive (e.g., Durlach, 1989; Farley, 1987), even "exquisitely sensitive" (Colwill, Absher, & Roberts, 1988, p. 4434), to the CS-US contingency, and that "it is CS-US contingency, rather than simple contiguity, that promotes conditioning" (Rescorla, 1986, p. 391). Rescorla's 1967 paper, in which he recommended the "truly random" control procedure, also continues to be cited frequently (49 times from 1986 through mid-1989), while the 1972 paper in which he withdrew that recommendation and renounced contingency theory has been cited less frequently (16 times in

the same period). Rescorla himself reverted thereafter to the traditional unpaired procedure, as Gormezano and Kehoe (1975) have noted, but the random procedure continues to be used rather widely. Miller and Matzel (1989), who praise contingency theory as "an elegant and largely successful effort to correct contiguity theory's failure in handling unsignaled USs" (p. 65), judge the "truly random" procedure to be "one of the better controls," despite their reservation that "extraneous factors" may occasionally permit "the perception of one probability to be completed in fewer trials than the other" (p. 64).

For Rescorla, the assertion that "it is the contingency between CS and US which matters for the development of an association" is an empirical generalization rather than a theoretical postulate—"a global view" rather than a "molecular account" such as the Rescorla-Wagner theory provides both of "supporting" data and data "at variance with that view" (1985, pp. 116-117). The data "at variance" are all too often ignored. "Conditioning occurs to the extent that  $p(\text{US}/\text{CS})$  exceeds  $p(\text{US}/[\text{no}]\text{CS})$ ," Rescorla (1988) tells neuroscientists on the basis of his 1968 results, but he looks for "explanation" to "a more complex version of contiguity" (pp. 342-343). The same distinction was made several years ago by Mackintosh (1983), who rejected contingency theory and went on to truncate the empirical generalization, denying that contingency is sufficient for conditioning while continuing to believe that it is necessary: "Excitatory and inhibitory conditioning do appear to require positive and negative contingencies respectively" (p. 182), he concluded, but even that diminished claim has long been untenable, contradicted by much of the same evidence that prompted the rejection of contingency theory as theory.

Two main lines of research bear on the question of whether a nonzero CS-US contingency is necessary for conditioning. One is a series of aversive conditioning experiments beginning with Rescorla's in 1966 and 1968. The second (somewhat special features warrant its separate consideration) is a parallel series of appetitive conditioning experiments begun a few years later by Gamzu and Williams (1971, 1973). Both provide abundant evidence of conditioning when the probability of reinforcement is the same in the presence of a CS as in its absence and suggest that the concept of contingency might be dispensed with entirely in the analysis of classical conditioning.

### Aversive Experiments

The conclusion from Rescorla's 1966 and 1968 experiments that contingency is necessary for conditioning was contradicted by the results of subsequent experiments that showed conditioning both of the CS and the context in the course of random training. As noted earlier, differential response to the CS after one and after three sessions of random training, although not after six sessions, was found in some follow-up work by Rescorla (1972) himself, who explained the results in terms of his and Wagner's new contiguity theory. A few accidental pairings of a salient CS with shock—that is, pairings scheduled by the randomizing devices to which control of stimulation had been relinquished—would be enough to make the combined associative strengths of CS and context substantially greater than that of the context alone. Ultimately, however, the associative strength of the context would increase to the point at which the

combination overpredicted the US, and the associative strength of the CS would begin to decline. How long that would take depended, according to the theory, on such variables as the salience of the CS, the rates of growth and decline in associative strength with reinforcement and nonreinforcement, and the intertrial interval.

Kremer (1971) and Kremer and Kamin (1971) had already reported differential response to the CS after 4 and 5 sessions of random training even in animals tested for the first time 20 days later, which seemed to rule out a nonassociative interpretation. Benedict and Ayres (1972), whose animals had 2 sessions of random training, stressed the importance of "the first few events" (p. 327) that chanced to occur in those sessions; the CS proved to be excitatory if the events were CS-US pairings, but not if they were nonpairings, even though the CS-US contingency in the training period as a whole was zero. Of the nonpaired events, Ayres, Benedict, and Witcher (1975) found the more important to be the USs, the effects of which could be attributed to contextual conditioning. Keller, Ayres, and Mahoney (1977) reported that differential response to the CS, which they found in tests made after as many as 18 sessions of random training, tended to be reduced by unsignaled USs over the entire course of that training. That accidental backward as well as forward pairings may have associative consequences—in the backward case, inhibitory—was reported more recently by Wagner and Larew (1985). After 5 sessions of random training, there was more response to the CS in a group for which backward pairings had been gated out than in a control group for which the total number of USs was the same. It should be clear from these experiments that what is important for conditioning is not the CS-US contingency, which was zero in every case, but the particular series of events—CSs and USs in a defined context—to which the animals are exposed.

The reality of contextual conditioning in random training was recognized implicitly by Rescorla (1968) in the design of his conditioned-suppression experiments. After the rats had been pretrained to press a lever for food, there were off-baseline conditioning sessions without opportunity for lever pressing. Then, before the effect of the CS on lever pressing was tested, there were some baseline recovery sessions in which neither CS nor shock was presented. If the suppressive effect of the CS on lever pressing was to be measured, there had to be some, but lever pressing was suppressed by fear of the context developed during random training and reappeared only as that fear was extinguished in the recovery sessions. Working at about the same time as Rescorla and in the same laboratory, Seligman (1968) reported that rats not only stopped lever pressing completely but developed stomach ulcers in the course of prolonged on-baseline random training, which he attributed to chronic fear; rats given paired training involving the same number of shocks showed less disruption of lever pressing and developed no ulcers. Contextual conditioning was measured by Kremer (1974) in terms of the rate of lever pressing in baseline recovery sessions given after one, three, or five sessions of random training; the greater the amount of random training, the more marked the suppression of lever pressing, he found, and the slower the recovery. When Rescorla (1972) signaled shocks given in the absence of the CS with another salient stimulus (a technique expected on the basis of Rescorla-Wagner theory to minimize

contextual conditioning), response to the CS was enhanced (as expected) even though the probability of shock in its presence and absence was the same.

Contextual conditioning in random training is not in itself incompatible with the assumption that contingency is necessary for conditioning if the probability of shock in the training situation is compared with the probability of shock outside the situation (Quinsey, 1971). By the same token, however, the CS also should be conditioned, although it was a lack of differential response to the CS after contextual extinction that seemed to establish the necessity of contingency for conditioning in the first place. Dweck and Wagner (1970) measured suppression of drinking in two groups of rats that had been given random training, one with interpolated sessions containing neither CS nor US and the other without such sessions. Differential response to the CS was found in the first group but not in the second even though there was no difference between them "in the correlation between CS and US occurrence as computed over [the subject's] entire daily experience. What did differ was the degree of correlation . . . as computed over the total time spent in the experimental chamber" (p. 147), by which computation the animals trained without interpolated recovery sessions should not have been fearful either in the presence or in the absence of the CS, because the two probabilities of reinforcement were the same. What Dweck and Wagner found was fear of the context but no differential response to the CS after baseline recovery. Neither method of computing contingencies was adequate to all of the data.

Soon there was evidence, too, that random training may retard conditioning of the CS in subsequent paired training. Kremer (1971) found positive transfer after four sessions of random training with a relatively high density of stimulation (the CS producing strong suppression from the outset) but negative transfer after 20 sessions of random training with a schedule of lower density that involved fewer accidental pairings. Negative transfer was reported also by Mackintosh (1973). The simplest interpretation is that the negative transfer was due to the combined effects of experience with unreinforced CSs and experience with unsignaled USs, each of which is known to retard acquisition. The CS-preexposure or latent-inhibition effect has been attributed sometimes to loss of salience (Reiss & Wagner, 1972; Rescorla, 1971) or attention (Lubow, 1973) and sometimes to the development of inhibition (Kremer, 1972). The US-preexposure effect (Randich & LoLordo, 1979) has been attributed primarily to contextual conditioning and blocking. The role of contextual conditioning in all three effects—the negative transfer from CS preexposure, from US preexposure, and from random training with both stimuli—is indicated by the fact that all three are context specific (Matzel, Schachtman, & Miller, 1988), which means that they are reduced when the initial training and the acquisition tests are carried out in different contexts. Researchers have suggested that something more (called *learned irrelevance*) is involved in the negative transfer from random training, the evidence for which is not entirely convincing (Baker, 1976; Dess & Overmier, 1989; Matzel et al., 1988), but what the transfer results continue to show is that a good deal of conditioning takes place during random training.

Remaining to be considered on the topic of aversive training is a shortcoming of Rescorla's 1966 and 1968 experiments that

was not widely appreciated until work on random appetitive training had begun (Gibbon, Locurto, & Terrace, 1975). To test the hypothesis that CS-US contingency rather than CS-US contiguity is responsible for excitatory conditioning, Rescorla used two methods, neither of them satisfactory. One was to keep the number of CS-US pairings the same while adding enough unsignaled shocks to bring the contingency to zero. For example, animals trained with  $p(\text{US/CS}) = .4$  per 2-min interval and  $p(\text{US/noCS}) = 0$  would receive 4.8 shocks on average during a 2-hr training session in which there were twelve 2-min presentations of the CS, and animals trained with  $p(\text{US/CS}) = p(\text{US/noCS}) = .4$  would receive 5 times as many shocks (24 in all). The second method was to redistribute the shocks, which kept shock density the same but reduced both the number of CS-US pairings and  $p(\text{US/CS})$  for the random group.

The scant evidence available on the role of shock density in aversive training is inconclusive. In the only relevant random experiment, Rescorla (1972) found better response to the target CS in a group for which the added shocks were signaled by another stimulus than in a second group for which they were unsignaled, but considerably less than in a third group for which there were no added shocks. Later, two signaling experiments with  $p(\text{US/CS}) = .4$  and  $p(\text{US/noCS}) = .2$  were reported. Rescorla (1984) took great pains to minimize contextual conditioning in one group of animals by prolonging their training and signaling shocks in the absence of the CS with a stimulus that itself had an even longer history of pairing with shock. Differential response to the CS was almost (although not quite) as great in those animals as in animals to which no shocks were administered at all in the absence of the CS. Jenkins and Shattuck (1981) trained a group of animals with the same density of shock as in a .4-.2 group but with all the shocks signaled by the CS, which changed the contingency to .4-0. Differential response to the CS was not enhanced by the treatment. Whatever their implications for the role of shock density, the results of none of the three experiments support the assumption that CS-US contingency is important for conditioning. In the first two, response to the CS improved while the contingency remained the same; in the third, response to the CS remained the same while the contingency became more positive.

### Appetitive Analogues

Soon after Rescorla's early work on aversive conditioning in dogs (1966) and rats (1968), some analogous work on autoshaping in pigeons was reported, with results that were much the same and that seemed to support contingency theory. Gamzu and Williams (1971, 1973) found that pigeons do not learn to peck a lighted key paired with food if food is presented with the same probability when the key light is off as when it is on, and pigeons trained with a positive contingency stop pecking the lighted key when the contingency is reduced to zero by the addition of unsignaled food. At first glance, these results may seem more damaging to simple contiguity theory than the aversive conditioning results because the measure of performance in autoshaping experiments is absolute rather than relative. If (to paraphrase Estes, 1969, on Rescorla) the pigeons learn in random training to anticipate food equally in the presence and absence of the keylight, then, even though the dark key provides

an insufficient outlet for that anticipation, they might be expected to peck the lighted key. It is conceivable, however, that frequent reinforcement in the absence of the keylight establishes some other response, such as orientation to the feeder, which is incompatible with orientation to the key and with key pecking (Boakes, 1977). Competing orientations have been measured directly by Hearst et al. (1980).

A troublesome feature of this work, which has already been considered, is that the density of reinforcement must be sharply increased if the CS-US contingency is to be reduced to zero while the number of pairings remains the same. Gamzu and Williams (1973) used 32-min training sessions in which there were 12.9 reinforcements on average when the CS-US contingency was positive and 57.9 (almost one every 30 s) when it was zero. In an experiment by Gibbon et al. (1975), comparable increments in US density impaired the acquisition of key pecking in training with a positive contingency even when all the added reinforcements were signaled by the same keylight. Jenkins, Barnes, and Barrera (1981) found poor acquisition of key pecking when reinforcements were given every 40 s or so, whether all or hardly any of them were signaled by the CS; acquisition was excellent with 1 signaled reinforcement per session but poor with 30 signaled reinforcements. A suppressive effect of US density per se may be inferred from the results of an experiment by Lindblom and Jenkins (1981): After a series of autoshaping sessions followed by a series of sessions in which the contingency was reduced to zero by the addition of unsignaled USs, the pecking, which had stopped, reappeared in unreinforced tests with the CS alone. These results, replicated recently by Rescorla (1989), are instructive also because they contradict the contingency view of random training as an extinction procedure—a procedure for studying “the loss of contingency-dependent learning” (Rescorla, 1967, p. 75).

Appetitive conditioning under circumstances in which the CS-US contingency is zero has been demonstrated in several experiments. Durlach (1983) autosshaped three groups of pigeons, a conventional group (with no added USs), a random group (with added USs), and a third group for which the added USs were signaled by a tone designed to minimize contextual conditioning. Key pecking developed in the third group even though the probability of reinforcement was the same in the presence and absence of the keylight (although its performance was considerably inferior to that of the first group, for which the density of reinforcement was much less). Similar results have since been reported by Goddard and Jenkins (1987). In another experiment by Durlach (1982), pigeons were autoshaped with one CS in one context and (concurrently) given random training with a second CS in a second context. Tested subsequently, the animals responded more to the second CS in the first context than in the second context, suggesting that poor response in the second context was in part at least a performance effect rather than an associative effect. Whatever the role of contextual conditioning in random appetitive training, about which there is considerable disagreement in the contemporary literature (Balsam & Gibbon, 1988; Durlach, 1989; Goddard & Jenkins, 1987; Rescorla, 1989; Robbins, 1988), the fact remains that there is clear evidence of conditioning, both of the CS and the context, when the probability of reinforcement in the presence and absence of the CS is the same.

An alternative to the conventional discrete-trials autoshaping procedure is provided by the variable-time (VT) procedure of Gamzu and Schwartz (1973), who exposed pigeons to two-component multiple (*multi*) schedules of response-independent food. With the color of the keylight sometimes red and sometimes green, the pigeons responded differentially when one color was reinforced every 33 s on average and the other color never was reinforced (*multi* VT 33-s EXT), but they responded hardly at all to either color when the probabilities of reinforcement were the same (*multi* VT 33-s VT 33-s)—that is, when the probability of reinforcement was the same in the presence of each stimulus as in its absence. A decided advantage of this procedure is that, in it, noCS gives way to a second, clearly defined stimulus of the same salience and duration as the target CS. Density of reinforcement and contingency still are confounded when two components are used, but they can be unconfounded by adding a third component. Farley (1980) and Brandon (1981) used three key colors to which their pigeons responded differentially on *multi* VT 30-s VT 60-s EXT (most to the 30-s color; least to the EXT color) but essentially not at all on *multi* VT 60-s VT 60-s VT 60-s. Although the latter result may appear to support the assumption that contingency is necessary for conditioning, the two together do not. With the probability of reinforcement the same in its presence as in its absence, there should be no conditioning of the 60-s color in the 30-60-EXT case, unless, of course, the probability of reinforcement outside the experimental situation is included in the computation and assumed (properly enough) to be low, but then there should be conditioning also in the 60-60-60 case. If the outside world is excluded, there should be no response to the common 60-s color in either case. Whether the outside world is included in the computation and, if it is, whatever the probability of reinforcement assigned to it, the contingency rule requires that response to the common 60-s color should be the same in the 30-60-EXT case as in the 60-60-60 case.

Although pigeons show very little key pecking in *multi* VT training when the probability of reinforcement in each component is the same, the effect is not a general one. Brandon, Satake, and Bitterman (1982) found that autoshaped goldfish respond differentially to two colors on a *multi* VT 60-s EXT schedule and to three colors on *multi* VT 30-s VT 60-s EXT but at an intermediate level when all colors are on VT 60-s, and they replicated their three-component appetitive results in an aversive conditioning experiment with shuttling as the response and brief shock as the US. Results like those for key pecking in pigeons were obtained by Boakes (1977), who studied lever pressing in rats with illumination of the lever as the CS, but quite different results were obtained by Farwell and Ayres (1979), who studied classically conditioned head poking in rats (insertion of the head into the aperture of a sucrose feeder) with illumination of the aperture as the CS. Farwell and Ayres found frequent head poking both in the presence and the absence of the feeder light when the probability of reinforcement was the same. These different results may be traced to structural differences in the training situations. In the key-pecking and lever-pressing situations, the animal responds to a CS that is spatially discontiguous with the US, giving rise to the possibility, as Boakes (1977) suggested, that orientation to the locus of the US competes with orientation to the CS. There seems to be no such

competition in the head-poking situation or in the target-striking situation used for goldfish, where CS and US are presented at the same place, or in the goldfish shuttle box, where the stimuli are not sharply localized. What is important for present purposes, in any case, is that the contingency rule brings no order to the VT results.

In appetitive as well as in aversive conditioning, acquisition is retarded when training with a positive contingency is preceded by random training, and asymptotic performance also is depressed. Gamzu and Williams (1973) reported very little key pecking by pigeons in 35 autoshaping sessions given after 14 sessions of random training. The retardation effect, if not the less understood asymptotic effect (Brandon & Paul, 1987), may reduce in large part to the CS- and US-preexposure effects, and like them is context specific. In an autoshaping experiment with pigeons by LoLordo, Fairless, and Stanhope (1985), unreinforced exposure to the context interpolated between random training and training with a positive contingency enhanced acquisition. In an experiment by Tomie, Murphy, Fath, and Jackson (1980), two groups of pigeons were only feeder trained, and two other groups were given random training in one context, after which all were trained with a positive contingency either in the same or a different context; the retardation effect was sharply attenuated by change in context. Tomie (1976) found that autoshaping to a keylight in the same context, but not in a different one, was impaired by prior random training even with a different CS (a tone). Evidence of an association between CS and context in random training has been reported by Kaye, Preston, Szabo, Druiff, and Mackintosh (1987), who studied conditioned approach to water. After concurrent random training with one CS in one context and a second CS in a second context, acquisition was retarded in positive-contingency training with each CS in the same context as before, but not with CSs and contexts interchanged. In the face of the transfer results, maintaining a belief in the associative neutrality of random training would seem rather difficult.

### Other Considerations

Although our question thus far has been whether contingency is necessary for conditioning—to which an unqualified negative answer must, of course, be given—it may be instructive to consider also the question of sufficiency. Phenomena such as overshadowing and blocking suggested clearly at about the time Rescorla's theory was introduced that a positive contingency is insufficient for excitatory conditioning, but soon there was evidence that went further. In conditioned-suppression experiments by Rescorla (1970) and Wagner (1971), which were inspired by their new contiguity theory, previously acquired excitation was lost in the course of training with a positive contingency. After two stimuli had been separately reinforced, reinforcing them together reduced response to each of them alone in subsequent tests. The magnitude of the decrement was found to increase with the salience of the stimuli (Kamin & Gaioni, 1974) and with the amount of compound training (Kremer, 1978). In an even more dramatic experiment by Kremer (1978), a third stimulus, added during the compound training and positively correlated with reinforcement, as were the others, was shown by subsequent summation and retarda-

tion tests to have become inhibitory. If a positive contingency is not sufficient for excitatory conditioning, neither is a negative contingency sufficient for the development of inhibition. Several experiments have shown that a CS negatively correlated with shock fails to become inhibitory if the shock is signaled by a different stimulus (Baker, 1977; Duss, Patterson, & Overmier, 1986; Fowler, Kleiman, & Lysle, 1985), and under other circumstances, a CS that is negatively correlated with reinforcement may even become excitatory. As Rescorla (1972) noted early, the fact of second-order excitatory conditioning is "blatantly inconsistent" with the contingency rule because the second-order CS "predicts the absence of the US" (p. 42).

An interesting question is why the idea that conditioning depends on contingency should have continued to find wide acceptance in the face of so much contrary evidence. We have referred already to its congruence with the cognitive and pseudoevolutionary thinking of recent decades and to the indelible influence of Rescorla's 1966 and 1968 results taken at face value and in isolation. Important, too, is a certain blurring of distinctions between the contingency view and Rescorla-Wagner theory, based perhaps on common authorship and broad structural similarity, which has permitted the one to profit from the achievements of the other. The success of Rescorla-Wagner theory in accounting for Rescorla's 1966 and 1968 results has seemed somehow to validate the original interpretation of them and to create a spurious impression of compatibility. To say, as has been said, that the Rescorla-Wagner theory was "designed to explain the effects of variations in contingency" (Mackintosh, 1983, p. 280) or that it "provided an explanation for the dependence of conditioning on contingency" (Jenkins & Shattuck, 1981, p. 159) certainly is misleading. There is no dependence of conditioning on contingency qua contingency, nor was the Rescorla-Wagner theory predicated on the assumption that there is, and many of the results that contradict the contingency view can be understood in terms of the Rescorla-Wagner theory, which suggested many of the experiments that produced those contradictory results in the first place. For a more complete answer to the question, we look to philosophers and historians of science, who might find much of value in entertaining it.

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